An Integrated Modeling Approach To The Surface and Subsurface Flow Processes Of The Bay−Delta System

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Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0215: An Integrated Modeling Approach To The Surface and Subsurface Flow Processes Of The Bay−Delta System

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

Summary: The goals of the proposed project are to modify and refine two existing models for coupled groundwater−surface water flow that are currently used in California. The proposed modifications are based on user feedback and identified development issues. The modifications may be valuable to users and decision makers, and the proposed approaches are generally sound, but in some instances lack sufficient detail for evaluation of feasibility and value. Moreover, the extent to which the two models are used (and thus the need for this project) is not fully demonstrated and difficult for outside reviewers to judge. This is an ambitious project with numerous elements and an extensive budget, however the relative importance, value, and need for the various elements are unclear. For example, is a given model modification or ensemble set of modifications expected to have a significant impact on either prediction accuracy or management decisions, and are all elements of the proposal needed? The technical reviewer ratings of this proposal were good, fair, and very good. However, the third review was discarded because it was non−critical and had little substance. The remaining technical reviewers were in agreement with the primary panel review. Goals: The overall goal and objective of refining the two models are clearly stated, and the objectives of the numerous

tasks are fairly well described. However, there are no clearly stated hypotheses. Justification: The project is generally justified in terms of the need for improving predictions for decision makers. However, some of the specific tasks are not well justified in terms of their importance and value either for model performance or environmental/societal needs. For example, more explicit justification of the need/value for improving the spatial resolution of the model (Task 2, Phase II) and including wetland operations (Task 5, Phase II) would be useful. More importantly, what is the relative and absolute importance of the various model modifications and are they all needed? The authors propose an ambitious, expensive project that would benefit from more detailed justification of the individual or ensemble model modifications. Finally, the extent to which these models are used and their importance to the state could be better explained for outside reviewers to help justify the costs and value of this project. Approach: The approaches specified for each task are generally sound, but some lack detail, making their feasibility and likelihood of success difficult to assess. For example, the approach for incorporating reservoir operations is vague (Task 3, Phase 1), as are many aspects of the surface flow simulations (Task 1, Phase 1) (how many surface flow models will be considered, what are the selection criteria, what data are required to implement and test them?). Feasibility: The project is generally feasible, but feasibility and likelihood of success for some tasks are uncertain, as discussed above. Monitoring: Not applicable. Products: The primary product is refined and improved model results that are expected to benefit decision makers and resource management. However, the magnitude of this benefit is unspecified and difficult for reviewers to assess. Capabilities: An extensive group of qualified PIs has been assembled for this project and should ensure timely completion of the work and general success. Budget: The budget is reasonable for the size of the project and number of investigators, but it is unclear that all of the tasks and associated PIs are needed. Reviewer 2 considers the budget overweighted by consultant fees.

Additional Comments:

Summary: The goals of the proposed project are to modify and refine two existing models for coupled groundwater−surface water flow that are currently used in California. The proposed modifications are based on user feedback and identified development issues. The modifications may be valuable to users and decision makers, and the proposed approaches are generally sound, but in some instances lack sufficient detail for evaluation of feasibility and value. Moreover, the extent to which the two models are used (and thus the need for this project) is not fully demonstrated and difficult for outside reviewers to judge. This is an ambitious project with numerous elements and an extensive budget, however the relative importance, value, and need for the various elements are unclear. For example, is a given model modification or ensemble set of modifications expected to have a significant impact on either prediction accuracy or management decisions, and are all elements of the proposal needed? The technical reviewer ratings of this proposal were good, fair, and very good. However, the third review was discarded because it was non−critical and had little substance. The remaining technical reviewers were in agreement with the primary panel review. Goals: The overall goal and objective of refining the two models are clearly stated, and the objectives of the numerous tasks are fairly well described. However, there are no clearly stated hypotheses. Justification: The project is generally justified in terms of the need for improving predictions for decision makers. However, some of the specific tasks are not well justified in terms of their importance and value either for model performance or environmental/societal needs. For example, more explicit justification of the need/value for improving the spatial resolution of the model (Task 2, Phase II) and including wetland operations (Task 5, Phase II) would be useful. More importantly, what is the relative and absolute importance of the various model modifications and are they all needed? The authors propose an ambitious, expensive project that would benefit from more detailed justification of the individual or ensemble model modifications. Finally, the extent to which these models are used and their importance to

the state could be better explained for outside reviewers to help justify the costs and value of this project. Approach: The approaches specified for each task are generally sound, but some lack detail, making their feasibility and likelihood of success difficult to assess. For example, the approach for incorporating reservoir operations is vague (Task 3, Phase 1), as are many aspects of the surface flow simulations (Task 1, Phase 1) (how many surface flow models will be considered, what are the selection criteria, what data are required to implement and test them?). Feasibility: The project is generally feasible, but feasibility and likelihood of success for some tasks are uncertain, as discussed above. Monitoring: Not applicable. Products: The primary product is refined and improved model results that are expected to benefit decision makers and resource management. However, the magnitude of this benefit is unspecified and difficult for reviewers to assess. Capabilities: An extensive group of qualified PIs has been assembled for this project and should ensure timely completion of the work and general success. Budget: The budget is reasonable for the size of the project and number of investigators, but it is unclear that all of the tasks and associated PIs are needed. Reviewer 2 considers the budget overweighted by consultant fees.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The team has the capability to perform the proposed tasks, and the proposed work is feasible (though the likelihood of success for some tasks is uncertain due to insufficient detail of methods.). However, the goals of the proposed work do not address specific scientific hypotheses. Therefore, the proposed work, though worthwhile, would have uncertain scientific value. In addition, the proposal would have been stronger if it contained a more detailed description of the proposed modifications to the models, and more extensive justification of the need for the proposed improvements (and the value of the resulting benefits). Finally, the panel questioned whether Calfed should support the maintenance and modification of other groups' models.

Technical Review #1

proposal title: An Integrated Modeling Approach To The Surface and Subsurface Flow Processes Of The Bay−Delta System

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The general project goals of the project are well−stated, although specifics are often lacking. While the IGSM2 and CVGSM3 models have widespread application to the San Joaquin hydrologic system, they are not extensively used in other settings thereby making the goals appear somewhat limited to hydrologists who are not working in this geographic location. This drawback is not helped by a proposal which shows the "extent of the CVGSM3 model domain" to be the entire state of California (Figure 1) and the grid mesh of CVGSM3 (Figure 2) contains no scale or reference point. To make a convincing case for the proposal, better care could have been taken in educating reviewers.**
Rating	**good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**Understanding the justification for the project is hampered by a severe lack of scientific background for the project. References to published scientific**

	literature are non-existent. (The six references in the proposal are internal California State reports, one personal communication, and one internal university report). The "Justification" section of the proposal (p. 12) contains references to Division of Water Resources publications not listed as references. The proposal is loaded with acronyms that people outside this project have no experience with nor understanding of. Examples include the WEAP model (top of p. 9), the Comp Study of the USACE (bottom of p. 9), DWR's DPLA (middle of p. 10) and CWEMF (top of p. 12). For these reasons, any scientist attempting to evaluate the justification, approach, and feasibility of the project is at a severe disadvantage relative to those with extensive experience in the California DWR system.
Rating	**good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**Approaches outlined in the proposal are numerous and ambitious. Most seem sound although some of the proposed methods did raise red flags with me. Attempting to represent a domain as geologically complex as the Great Valley of California with three stratigraphic layers is obviously too simplistic (as the PIs point out) and that aspect of the model needs to be improved. However, there are indications that the PIs do not appreciate how difficult it can be to accurately represent stratigraphy in a domain as vast as this one. My experience is that attempting to understand stratigraphy from public water well logs is a fool's errand: there is simply no QA or QC for the dozens of water well drillers who are required by law to provide that information to state agencies but who**

	have no interest in nor training for the task. Also, attempting to quantify evapotranspiration on the basis of crop types and meteorological data (Phase II, Task 4) is a difficult task at best and the proposal should contain references to methods that have been successful in other settings.
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The project is probably feasible given the capabilities of the PIs and the very large budget requested (as discussed elsewhere in the review). Given the large number of tasks and the limitations of the proposal size, it is difficult to assess whether all of the tasks are technically feasible, but my sense is that most are.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Monitoring the progress of upgrading complicated software is somewhat problematic. However, the proposal includes a pre− and post peer review of the CVGSM3 software and this constitutes an important and useful monitoring task for the proposed work.**
Rating	**excellent**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**The products produced by the proposed project are extensive and worthy additions to the tools available to water managers in the Central Valley of California.**
Rating	**excellent**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The extraordinary number of investigators involved in the project (21) makes it difficult to assess the overall capability of the investigative team. Although all tasks in the project are assigned to individuals in the budget section of the proposal, it is very difficult to evaluate how individual investigators will perform specific tasks. To the extent that I was able to evalute the individual investigator's capabilities, they do seem like competent scientists and engineers who would accomplish the work.**
Rating	**very good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	

	Although this is a very ambitious proposal, the budget ($2.3 million) seems high for the products that will be generated. With 21 separate investigators involved, the bill adds up in a hurry. For many of them, this project appears to constitute a significant fraction of their annual salary. While the goals of the project are worthy, I have doubts that the cost/benefit of the effort is favorable although that may be a judgment better left to the program managers.
Rating	**good**

Overall

Provide a brief explanation of your summary rating.

Comments	**This is an ambitious and expensive proposal. The goals appear worthy and thus it is disappointing to find that the PIs did not provide more scientific background and explanation for potential reviewers. With a couple of exceptions, the approaches appear to be sound. The results and products may or may not justify the project's costs. The PIs seem qualified and probably capable of producing the desired results.**
Rating	**good**

Technical Review #2

proposal title: An Integrated Modeling Approach To The Surface and Subsurface Flow Processes Of The Bay−Delta System

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals to develop an integrated modeling of the surface and subsurface flow processes of the Bay−Delta system, the objectives (improvements of IGSM2 and CVGSM3), and the hypotheses (issues arose during development and application) are clearly stated and internally consistent.** **The idea is timely and important to the water community in Bay−Delta region.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The study is justified relative to existing knowledge of surface/subsurface flow processes.** **The conceptual model is clearly stated in the proposal and it explains the underlying (hydrogeological and hydrological) basis for the proposed work.** **The selection of research project with the full−scale implementation is justified.**

Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The approach is well designed and appropriate for meeting the objectives of the project and is feasible.** **The results are likely to add to the base of knowledge.** **The project is likely to generate novel information and improvement of methodology or approaches of modeling.** **The information will ultimately be useful to decision makers.**
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach is fully documented and technically feasible. However, more detailed discussions of methods in Phase I Task1 and solver in Task2 will be helpful.** **The likelihood of success is high.** **The scale of the project is consistent with the objectives and within the grasp of authors.**
Rating	

	good

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**Monitoring is not proposed.** **There are plans to interpret/evaluate monitoring/observation data for the modeling purpose.**
Rating	**good**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**Products of value are very likely from the project.** **Contributions to larger data management systems are relevant and considered, but integration to these systems is not stated clearly although interaction with databases such as HEC−DSS, SQL, Access, and others are mentioned.** **Interpretive (or interpretable) outcomes from IGSM2 and CVGSM3 are very likely from the project.**
Rating	**very good**

Additional Comments

Comments	**Would be interesting if all model/data/results of IGSM2 and CVGSM3 is posted in the internet.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The track record of authors in terms of past performance is excellence and impressive. Kadir, Dognul, and Moncrief are fully qualified to lead this project.** **The project team is qualified to efficiently and effectively implement the proposed project.** **They have available the infrastructure (mainly computers) and other aspects of support necessary to accomplish the project.**
Rating	**very good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget is reasonable and adequate for the work proposed except some items (GIS GUI and matrix solver). It would save money if adopt free available GIS GUI and/or solver or to use commercial products such as ArcGIS and/or NAG/IMSL.**
Rating	**good**

Overall

Provide a brief explanation of your summary rating.

Comments	**The proposal's strengths are (1) IGSM2 will be improved systematically; (2) a large amount of data will be reviewed and an updated conceptual model will**

	be generated; and (3) CVGSM3 will be improved systematically and more useful to address water problems. **The weakness is all development based heavily on funding, not seriously considering saving money through using free available resources or commercial products for GIS and/or solver.**
Rating	**very good**

Technical Review #3

proposal title: An Integrated Modeling Approach To The Surface and Subsurface Flow Processes Of The Bay−Delta System

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals (as stated on the top of Page 3) are well−stated. Also, the Project Description is broken down into discrete phases and tasks, which provides an excellent structure to the problem. Unfortunately, the hypotheses and detail are lacking.**
Rating	**very good**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The motivation for the project is well documented. From reading the proposal, I get the sense that the end−user(s) of the modeling tool would like to obtain enhancements that improve the utility of the tool, and this proposal is intended to provide those enhancments.**
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to

generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**Through-out much of the document, the primary methodology proposed for achieving the objectives appears to focus on literature surveys, and a review of existing procedures. I find this approach to be fraught with dangers because there may not be an appropriate procedure for the intended enhancement. Thus it is not clear that a solution exists to the problem. I strongly urge the individuals involved to perform the literature review first, and then submit a proposal to implement the procedures. Funding a literature review is not consistent with my understanding of scientific research. If, however, the intent of this solicitation is to fund the training of staff scientists, then it may be an appropriate justification.**
Rating	**poor**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**As noted above, there is insufficient information provided to evaluate the likelihood of success. It is very possible that the proposed literature may provide the information required, but that is not clear at this point. I suggest that the alternatives be evaluated prior to the submission of the proposal, and the goal of the proposal is to evaluate the alternatives.**
Rating	**poor**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**There is consistent reference within the document to existing data records and peer−reviewed studies. Yet no formal evaluation methodology is provided. Clearly, a model with excellent calibration accuracy may not provide adequate predictive capabilities. Tradeoffs between model parameterization and model prediction accuracy are not addressed, nor are model evaluation criteria provided.**
Rating	**fair**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**If the project succeeds, the products will be of great value. I remain unconvinced, however, that the proposal will be able to accomplish the stated objectives. Also, while the products are indeed useful, equally important from a management perspective is the quantification of uncertainty, not only during the calibration phase but also for predictions. There are methodologies that provide this information, and users would benefit from these tools.**
Rating	**good**

Additional Comments

Comments	**Complex models suffer from a dependency on the assumed Model Construct, or Conceptual Model. Each conceptual model has its own suite of parameters and state variables. Using a**

	specific conceptual model may provide a different prediction than an alternative conceptual model. Relying on existing data to calibrate a model may result in reduced parameter errors, but may not reduce predictive errors due to parameter/state ambiguity and the use of an inappropriate conceptual model. Tools for addressing model performance are needed to provide management with the range of outcomes associated with alternative model constructs.

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The proposal assembles an enormous pool of talent. The accumulated experiences are clearly excellent. It is unclear, however, what methodologies and approaches will be employed, so that the overall effectiveness of the assembled pool is questionable. It may be that the proposed literature reviews will require additional expertise that has yet been identified. Also, the incorporation of model uncertainty may also require additional expertise.**
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**It appears that the bulk of the expenses will be allocated to external consultants. Many of these consultants will have large daily/hourly fees that extend over long periods of time. I am uncertain as to the wisdom of relying on a**

	few select individuals for the bulk of external consulting services. Perhaps a better strategy is to disaggregate the tasks into specific modules that can be individually funded using a competitive bidding process.
Rating	**fair**

Overall

Provide a brief explanation of your summary rating.

Comments	**This is a large and ambitious project. It is clear that the products will have great utility. Yet I have grave reservations on how the project is being proposed. There is a dependency on literature reviews that should already have been performed. There is also a large financial dependency on external consultants when these may not be needed. I suggest that this large proposal be disaggregated into smaller tasks with additional documentation for each task. I also suggest that building uncertainty estimation into the modeling effort will provide a more useful tool overall.**
Rating	**fair**